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Don't Scrap Education Yet

There Are a Lot of Loopholes in the Idea of Transplanting Memory by Means of RNA

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Science and Man

FOR SOME TIME, molecular biologists have been glancing sidewise at the human brain. They usually decide that its problems are still too complex to warrant a frontal attack with existing information and concepts. In the main, I have to agree with this and feel we must do much more drudge work in, for example, analyzing the composition of brain proteins before we can make exciting discoveries.

Some more adventurous souls have, however, attempted a grand leap and thus started one of the more confused controversies in recent scientific work. Without scientific corroboration or resolution of the issues, wide public interest has been attracted to "memory RNA," the proposition that memory is associated with changes in the composition of RNA molecules in the brain. In particular, the claim is that the memories of one animal (a flatworm or mouse) could be acquired by a second through the injection of RNA from the first.

RNA stands for ribonucleic acid as distinguished from DNA, deoxyribonucleic acid. It would be well to think of RNA, DNA and their third partner, protein, simply as proper names for the three fundamentally important substances of living cells.

PREFERRING a more systematic approach to brain mechanisms, I have had no personal experience with chemical memory substances. On more general theoretical grounds, however, the idea of memory RNA has puzzled me. Nothing in our firm knowledge of RNA biochemistry supports any detailed mechanism for it to function as a memory substance.

According to present knowledge, RNA molecules are faithful copies of a DNA blueprint. We would have to invent new mechanisms for the nerve cell to reimprint its own RNA and then read it out again.

Speculations like these might be useful guides to the choice of problems, but only experiment can answer questions about nature. Now we have a remarkable communique published in the journal Science quoting only negative results from experiments in several laboratories in their search for a memory substance.

Besides the fact of noncorroboration, we have the interesting problem of how and why the first experiments went wrong. Learning can only be tested by watching animals' behavior. And behavior is notoriously complex, alterable by the most subtle influences, such as could have an effect on, say, no the color of the solution in a test tube.

In fact, with rare and gratifying exceptions, controversies and false starts have been the general rule in experimental psychiatry, including the study of such crucial problems as the causes and treatment of schizophrenia. There are fundamental difficulties of experimental design which are widely known but insufficiently appreciated.

PROF. R. ROSENTHAL, Harvard psychologist, has specialized in the study of "experimenter-induced behavior" and shown many examples where the experimenter's personal expectations influenced the behavior of animal or human subjects, quite apart from the actual effect of the treatments. Just what nuances of handling or speech caused this is not always obvious. Most experiments on memory RNA can be faulted on this ground: they were not "double blind." A double blind experiment is one where one person makes up the experimental solutions and codes them. He should have no further contact with the animals; others must treat and test them. Thus no one who handles any animal may know which treatment was used.

Only when the experiment is completed are the codes unsealed. In practice, it is sometimes difficult to do a rigorous double blind and judgment must be correspondingly cautious. For example, the injections might have side effects on the animal, giving cues to the supposedly "blind" testers. In the course of an exciting exploration, it is humanly impossible not to speculate about which animals are which.

There are so many subtleties that it is usually impossible to judge only from the published reports just how rigorously they were done in the laboratory. The historic batting average of the scientist is what his colleagues may rely on. This must leave the layman even more baffled, since public visibility is not always correlated with the dispassionate objectivity of a given scientist.

THESE CRITICISMS may well make no difference to the public's impressions about memory RNA. We would all like to imagine such a substance and reality will only slowly dampen such hopes. But what are some more plausible expectations?

RNA is a vital constituent of nerve cells and, memory RNA or not, the more we learn of the triad of DNA-RNA-protein, the more we will learn about memory. We should also be hearing more about specific depressants and stimulants of memory function. Indeed, many familiar drugs probably already have important effects still not thoroughly explored. Nor should we slight what could be learned about suggestion and other psychological influences as factors in actual learning and educational experience.

Solid work in this field depends on an immense range of basic science, much seemingly irrevelant to human memory. The mathematical statistics of the rubber band, for example, are extremely pertinent to the chemical structure of RNA.

Undue pressure for prompt applications could erode not only the foundation of needed facts but the sobriety needed for turning speculation into sound policy. A more goal-oriented community of technicians might have leaped too soon at making a national goal of replacing education by a royal banquet of RNA.